

ZONE 3 COMMENTS

From: "Joe McGahan" <jmcgahan@summerseng.com>
To: "Margie Lopez-Read" <MLopez-Read@waterboards.ca.gov>, <mbjohnson@ucdavis.edu>
Date: 6/27/2007 1:14:49 PM
Subject: RE: comments on zone 3 report

It includes Westside comments. Joe Mc.

-----Original Message-----

From: Margie Lopez-Read [mailto:MLopez-Read@waterboards.ca.gov]
Sent: Wednesday, June 27, 2007 12:46 PM
To: mbjohnson@ucdavis.edu
Cc: Joe McGahan; mmtturner@ucdavis.edu; pklassen@unwiredbb.com; Susan Fregien; Bill Croyle
Subject: Re: comments on zone 3 report

Mike -

This will work, I just need to know who is represented in the comments.
Is it Westside and East SJ Coalitions?
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>>> "Michael Johnson" <mbjohnson@ucdavis.edu> 6/27/2007 12:32:48 PM >>>
Margie,

Parry and Joe are tied up in meetings today and requested that I submit the comments on the Zone 3 and Summary portions of the Monitoring data review.
Attached is the review. Do you need additional information like a cover letter, etc?

Mike

CC: <mmtturner@ucdavis.edu>, <pklassen@unwiredbb.com>, "Susan Fregien" <sfregien@waterboards.ca.gov>, "Bill Croyle" <wcroyle@waterboards.ca.gov>

Comments on RB Draft 2007 Zone 3 Review of monitoring data

General comments:

Throughout the document, the symbol for μ appears to be a u. It should be the former.

Page Z3-1. The presentation of the pesticide data by total pounds is misleading since a large portion of the applications are inert compounds that should not be included. The current description suggests that in some crops in some locations, between 100-150 lbs/acre of pesticides are applied. This does not take into account a product's water solubility, its relative toxicity to aquatic organism (if any) and whether applications of the products listed have the potential to reach waters of the state.

Page Z3-4, paragraph 3. In this paragraph and throughout the document, the focus is on the worst exceedances. For instance, the description of the fathead minnow tests starts with a statement that only 2 of 13 tests caused mortality above 50% but the remainder of the paragraph focuses on these two samples. There is no discussion of the remaining 11 tests or the level of mortality in those tests. In some tests, the survival of the minnows in the test water was at or above 90% that of the control samples; i.e. the death of a single minnow in a couple of the replicates could result in a statistically significant difference between the sample and the control. While statistically correct, the biological significance an 8% decrease in survival (for example) is questionable. The EPA manual addresses this issue but the ILP chose not to follow the manual in this regard. As a result very small differences in survival between controls and samples are treated in the same way as 0% survival. We recommend that the toxicity section should have an introduction that addresses the levels of toxicity and the differences in the interpretation of the results between the EPA manual and the ILP. Additionally, there should be some mention of those samples with very low mortality to balance the implication that levels of toxicity are severe. The same comment is applicable to both the *Ceriodaphnia* and the *Selenastrum* results.

Page Z3-4, paragraph 5. There is a statement that no correlations have yet been drawn between observed toxicity of fathead minnows and discharges. I believe that statement should be qualified by stating that "because of the small number of samples toxic to minnows within each monitoring program, sample sizes are too small to allow any correlations to be drawn between ...". Currently, the statement does not provide any reason allowing the conclusion to be drawn that the lack of correlation is because the monitoring programs do not want to understand what correlations exist.

Page Z3-5, paragraph 1 (first complete paragraph). We believe the first sentence should be deleted. The explanation later in the paragraph is sufficient to allow the reader to adequately assess whether the results of the toxicity were related to pesticides. But, because Phase II and Phase III TIEs were not performed on a majority of the samples, the definitive statement that pesticides are the cause cannot be made. The results of the Phase I TIE indicates that the cause of toxicity is a function of nonpolar organics which may be metabolically activated compounds, but this statement is consistent with pesticides as a cause, not definitive. If it was definitive, there would be no need to run Phase II and Phase III TIEs and those tests would not exist.

Page Z3-5, paragraph 2. At 3 locations in this paragraph, LC50 values are provided without attribution. There should be a reference provided for each, and if the species on which the LC50 was developed was not *C. dubia*, that should be noted as well. We are disputing the LC50 values, but all readers should be able to go back to the original work to review development of the value.

Page Z3-5, paragraph 2. The end of the paragraph reports that a series of pesticides detected at least once in Ceriodaphnia-toxic samples were not individually responsible for the toxicity (based on the LC50 values) but could have contributed to toxicity through additive effects, especially for samples in which non-polar organics were identified as the cause of toxicity. This statement can be interpreted as stating that all additional toxic samples had two or more chemical detections and toxicity was a result of additive or synergistic effects. This statement should be qualified by providing data on the number of toxic tests that also had 2, 3, 4, or more chemical detections, and the concentrations of the chemicals in the samples. Also, there has not been sufficient research performed to understand the additive or synergistic potential for all of the various combinations of these compounds in the samples. Qualifying the statement by stating that it is possible leaves out the qualification that it is also not possible. The reviewer should state what is known and can be supported scientifically, not what is interpreted as “possible.”

Page Z3-6, paragraph 5. The last sentence states that in samples with no algal toxicity, there were detections of herbicides that were at non-toxic levels or that antagonistic effects were in play. It's not clear what “in play” means, nor is it clear what “antagonistic effects” are. Both should be defined or explained thoroughly or the sentence should be deleted. It's clear that if herbicides were detected at levels below those known to reduce growth, and no reduced growth was observed in the toxicity test, the reduced levels of herbicides were insufficient to cause a reduction in growth. The implication in the last sentence is that they might have caused reduced growth but antagonistic effects prevented it. One of the implications is that nutrients may have stimulated growth which compensated for the effects of herbicides, but as stated in the next paragraph, the joint effects of nutrients and herbicides is not understood. Speculation that the RB understands the joint effects sufficiently to make the statement in the previous paragraph should not be included in this review.

Page Z3-7, paragraph 4. The first sentence should clarify what is meant by magnitude of certain hydrophobic pesticides. Does magnitude equate with concentration in the sediment? This section should also address the metals in the sediments.

Page Z3-8, first partial paragraph. There is a statement that says that in 33% of the toxic sediment samples, the cause could not be explained but could possibly be a result of other pesticides not measured but present in toxic amounts. This statement cannot be supported by the monitoring data and should be deleted. It is entirely speculative and not an interpretation of monitoring data.

Page Z3-8, paragraph 2. The first sentence indicates that DDT is still used in other countries, which is true but irrelevant to the current review. The beginning and ending clauses in that sentence are true.

Page Z3-12, Table Z3-4. This table addresses chlorpyrifos only and should be re-titled as such.

Page Z3-13, paragraph 2. The reference for the diazinon LC50 should be provided. Also, the water quality objective should be 0.16 µg/L, not 0.10 µg/L (*Amendments to the Water Quality Control Plan for the Sacramento River and San Joaquin River Basins for the Control of Diazinon and Chlorpyrifos Runoff into the Sacramento-San Joaquin Delta*, June 2006 Final Staff Report, pgs 25-26).

Page Z3-13, paragraph 3. The statement is made that DDT and DDE are trace contaminants in dicofol. DDT is used in the manufacture of dicofol and the EPA removed the registration when it appeared that the amount of DDT in the final product was too high. The registration was reinstated when it was demonstrated that a technical grade dicofol could be produced with a 0.1% (one tenth of one percent) DDT contamination level. DDE, a breakdown product of DDT, would not be expected to be found as a contaminant in the manufacturing process. Finally, given that the percentage of DDT in dicofol is 0.1%, the probability of finding DDT in a water body that was a result of contamination of dicofol would be very small, and would necessarily be accompanied by detections of dicofol in the water (chemical properties of the two compounds are similar). Dicofol was never detected suggesting that the detections of DDT and DDE were not a result of dicofol applications, but rather legacy applications of DDT during the last century.

Page Z3-16, first partial paragraph. The reference for the simazine growth effect should be provided.

Page Z3-16, paragraph 1. The discussion of dimethoate includes an objective based on a reference to 1/10 of the LC50 of a sensitive species. The reference should be provided as should the species used in the test. The test organism could be included parenthetically with no need for any additional text. This comment applies throughout the document to all uses of the 1/10 of the most sensitive species technique for developing a numeric objective.

Page Z3-17, paragraph 4. The preliminary report provided by the ESJWQC in November 2006 indicated that human fecal contamination was the most probable cause of the high coliform counts in surface waters. These results should be included in the current review because they are critical in the interpretation of the *E. coli* data submitted by the coalitions.

Page Z3-17, paragraph 5. This paragraph speculates on the potential causes of bacterial contamination but the speculations are not based on interpretations of the data. If the document is to be a review of monitoring data, this paragraph should be deleted.

Page Z3-19, first partial paragraph. The second line identifies Prairie Flower Drain and Hilmar Drain as the source of the majority of the EC/TDS exceedances on the east side of the river. It

should be pointed out that these sites are located very close to the SJR and overly a subsurface geology that is high in EC/TDS EC/TDS.

Page Z3-19, paragraph 3 under Summary section. No nutrient data were presented and this paragraph should be deleted. No exceedances of nutrients, with the exception of the single ammonia exceedance, have been reported to date and the tone of the paragraph is that nutrients are necessarily problematic, including a threat to human health, in Zone 3. No monitoring data are reported to substantiate these claims.

Section H: Closing Summary

General comment:

This section was difficult to understand. It seems to have multiple authors because the interconnections between sections and paragraphs are weak. For example, the 4th and 5th paragraphs on the first page are restatements of each other and one paragraph could be eliminated. Also, there appears to be several references to aspects of the ILP that were not included in the text of the monitoring report.

Page 2, paragraph 1. The last sentence indicates a summary of data gaps was previously discussed in the section, but no summary of data gaps was included. As a result, it is difficult to determine whether the four steps outlined below the paragraph will address data gaps.

Page 2, paragraph 3. The first sentence is unclear. It states that “There are constituents associated with irrigated runoff that will not be easily answered, and will require a concerted effort on the part of many agencies and groups, scientific studies, and perhaps the development of new management practices with different approaches to protecting water quality.” It’s not clear what this sentence is stating. The paragraph goes on to state that a CV Salinity Management Plan is being developed that will affect the ILP, but no details are provided.

Page 3, paragraph 1. The first sentence implies that if data represent a broad geographic area, management practices implementation is required. No justification for this statement is provided. The last sentence states that “To address the magnitude of this potential concern,” but it is not clear what the antecedent of “this” is, or what is meant by “potential concern”.

Page 3, paragraph 2. The first sentence states the obvious, and it would not be cost effective for any grower to implement management measures that had small or no potential to improve water quality.

Page 3, paragraph 3. It’s not clear what an “identified” time schedule is. How does this differ from a simple time schedule that identifies appropriate intervals?

Page 3, paragraph 4. The meaning of the statement “Construction of physical management practices may be one measure of implementation effectiveness” is unclear. The paragraph goes on to state that the ultimate measure effectiveness of management practices is improved water quality. However, it then states that since this may take many years to identify, it’s important to

measure management practice effectiveness through runoff or localized monitoring where appropriate. It appears that the paragraph is trying to state that the number of constructed facilities could be used as a surrogate for improved water quality based on the assumption that facilities such as sediment basins can reduce inputs to streams. While physical facilities may reduce inputs of specific constituents (e.g. sediment basins and sediment), the statement ignores numerous non-construction approaches to management of applied constituents that can be very effective in reducing inputs to streams. These approaches should not be ignored, nor should the Regional Board believe that only constructed management facility approaches can be effective.